

A Paradigm's Worth of Difference? Understanding the Impasse Over Modern Human Origins

SHELLEY L. SMITH AND FRANCIS B. HARROLD
*Department of Sociology and Anthropology, University of Texas
at Arlington, Arlington, Texas 76019*

KEY WORDS Multiregional Evolution; Recent African Origin;
philosophy of science

ABSTRACT The modern human origins debate within paleoanthropology has become polarized between two dominant models, Recent African Origin (RAO) and Multiregional Evolution (MRE). The debate has persisted and shows no sign of resolution despite the incorporation of new data and dates during the past decade. We examine the reasons for this stalemate, focusing on the presentation of these models by their principal advocates, Christopher Stringer and Milford Wolpoff. In particular, we consider whether the RAO-MRE dispute is a paradigm crisis. The modern human origins debate can be placed in the broader context of unresolved controversies within evolutionary biology (e.g., punctuated equilibrium vs. gradualism, use of cladistics, and species definitions). While the two sides hold conflicting views, we argue that such differences do not constitute a paradigm clash. Since both share a commitment to Darwinian evolutionary theory, the debate cannot be characterized as a paradigm clash at the level of, e.g., Ptolemaic vs. Copernican astronomy. Furthermore, we submit that a debate having historical roots reaching back into the previous century should not be portrayed as a conflict between competing paradigms in the Kuhnian sense. Preferences toward discontinuity or continuity wax and wane, persisting in a variety of scientific disciplines. We do not predict the quick demise of either MRE or RAO but are optimistic that careful evaluation of the characters and data on which claims about modern human origins are based will lead us toward a resolution of the current impasse. *Yrbk Phys Anthropol* 40:113-138, 1997. © 1997 Wiley-Liss, Inc.

As anyone even slightly familiar with the study of human evolution knows, the emergence of anatomically modern humans (hereafter, "AMH") from earlier, more archaic people has become the most salient and controversial issue in paleoanthropology. A large scientific literature and a flood of popular magazine articles and books testify that views on this issue have become polarized between two models. One, the Multiregional Evolution model (hereafter, "MRE"), claims that modern humans emerged gradually in each inhabited region of the Old

World from archaic populations in a single human species united by interregional gene flow (e.g., Wolpoff et al., 1984; Wolpoff, 1989a, 1989b, 1992; Frayer et al., 1993; Wolpoff et al., 1994a). In dramatic contrast, the Recent African Origin model (or "RAO") holds that all modern humans are descended from an African (or perhaps southwest Asian) population which, some time after evolving modern human form, spread over the Old World to replace regional archaic populations with limited or no interbreeding (e.g., Stringer and Andrews, 1988a; Stringer, 1989a, 1989b, 1992, 1993, 1994, 1995).

Despite significant new findings and interpretations [e.g., the Zafarraya Neanderthal find (Hublin et al., 1995) and the redating of important Middle Paleolithic Levantine fossil hominid sites (see Aitken and Valladas, 1993; Schwarcz and Grün, 1993)], no resolution is in sight. Competing hypotheses, of course, are essential to scientific progress; but they are expected to lead to evidentiary tests which should eventually show the explanatory superiority of one explanation over its rival(s). Thus in geology, plate tectonics came over time to be widely accepted, and in paleoanthropology, it gradually became evident that bipedalism had emerged before the great enlargement of the brain. Such progress, however, has proved elusive in the AMH origins issue.

In this paper we will examine the question of why this standoff persists. In doing so we will restrict our discussion primarily to the human fossil record and its interpretations. Although the fullest understanding of human evolution demands the synthesis of all relevant lines of evidence, this dispute is primarily about the fossil record. Thus the growing body of research in human mitochondrial and nuclear DNA relevant to AMH origins (e.g., D'Andrade and Morin, 1996; Ayala, 1995; Rogers and Jorde, 1995; cf. Lahr and Foley, 1994; Cann et al., 1994) will not be discussed here, both because DNA researchers differ among themselves over AMH origins and because RAO and MRE advocates alike say that their cases can stand independently of DNA evidence. Similarly, the Paleolithic archaeological record will not figure strongly in our discussion. Archaeological evidence of hominid adaptations and behavior is highly relevant to comprehending human evolution, but current understandings of this record are neither unambiguous nor easily applied to the AMH issue. Put very briefly, the major changes in human behavior betokened by the transitions to the Eurasian Upper Paleolithic and African Late Stone Age apparently occurred at least 50,000 years after the emergence of modern anatomy (e.g., Klein, 1995; Harrold, 1991). So far, claims of an earlier emergence of modern behavioral complexity (Deacon, 1989, 1993) have not been widely accepted, though recent work (Brooks

et al., 1995; Yellen et al., 1995) may change this picture.

We will not attempt a survey of all views on AMH origins, but will restrict ourselves to the two principal competing models, particularly as presented by their chief advocates, Milford Wolpoff for MRE and Christopher Stringer for RAO. These two skillful and experienced advocates, and the colleagues with whom they publish, operate in a polarized situation in which alternative or intermediate proposals between the extremes of MRE and RAO seem to receive little attention. Furthermore, as we will see, these competing models can be understood as the modern versions of two competing tendencies in the interpretation of the human fossil record that extend back into the 19th century.

WHY THE IMPASSE?

Possible answers to this question could point primarily either to shortcomings in the available database or to weaknesses in its interpretation. In regard to the first factor, paleoanthropology is in large measure a discovery-driven science. And a point properly stressed by many authors, notably Klein (1989, 1994, 1995), is that the fossil and archaeological record relevant to this dispute is still fragmentary, coarse-grained, and imperfectly dated, especially outside Europe. We should thus be unsurprised at our inability to test conclusively any models of AMH origins; the database, like a Rohrschach ink blot, can be harmonized with varying interpretations. Considering, for example, the uncertain stratigraphic provenience of crucial fossils like those from Jebel Irhoud, or the vague dating of Jinniushan and other Chinese fossils, or the lack of diagnostic human remains from the earliest Aurignacian in Western Europe, we hold that Klein's point has merit. One need not be a "rabid empiricist" to agree that new finds or firmer dates could readily clarify important questions and eliminate some possibilities.

However, this explanation is still partial. New data rarely "speak for themselves"; they must be interpreted, and MRE and RAO advocates have shown themselves to be adept at accommodating their models to

new findings. For example, while the surprisingly ancient thermoluminescence and electron spin resonance datings of the Qafzeh and Skhul remains were seen by the RAO camp as so many nails in the coffin of MRE, Wolpoff and his colleagues (Wolpoff, 1992; Frayer et al., 1993) retorted that 1) the Skhul-Qafzeh remains are not really modern, 2) they are not African in morphology, and 3) they are too early to fit RAO—and that therefore they help to disconfirm RAO! The available database may never improve to the point that either camp would be unable to accommodate its model to it.

If we must also look to the realm of interpretation to understand the impasse, the principal advocates in both camps have no doubt where the shortcomings lie: their opponents just don't get it (or worse, they *won't* get it). Each side has tested each model, and found its own clearly superior. However, their closed-minded opponents refuse to acknowledge the obvious. Thus, for example, Wolpoff et al. (1988:772) accuse Stringer and Andrews of "contradictions, misrepresentations, and omissions"; they "assume the hypothesis they set out to test . . ." and "do not appear to have examined [some relevant] fossils. . . ."

Similarly, Stringer and Bräuer (1994:416) take Frayer et al. (1993) to task for "misreading of the available data; selectivity of data used; and bias in the assessment of the merits" of the opposing models. They return in kind the accusation of assuming the hypothesis to be tested. The exchanges have sometimes taken on a nasty tone which does not contribute to the advancement of scientific understanding. While we think that the above charges have merit in certain cases, we do not think that either side has a monopoly on virtuous scientific procedure. Furthermore, we doubt that personal animosities are either insurmountable obstacles to progress, or the cause of the impasse; they are likelier to be its results.

A deeper interpretive problem is suggested by several authors (e.g., Willermet, 1993; Willermet and Clark, 1995; Lieberman, 1995; Lahr, 1994, 1996; Lahr and Foley, 1994) who have attempted to conduct systematic independent tests of one or both models. They note that RAO and MRE theo-

rists are to a significant extent talking past each other. These critics charge that in the debate, several crucial requirements are not consistently met. For instance, characters other than synapomorphies (shared, derived traits) have been used in inferring cladistic relationships; the possible role of developmental plasticity in the formation of phenotypic characters is often unknown; character states and polarities are inconsistently described; some "regional" traits turn out on examination not to be particularly characteristic of their regions; or the variables and fossils that best bolster a model are over-used for tests, thus biasing the sample. All these errors can result in misleading conclusions about evolutionary relationships.

These critiques underline a factor acknowledged by AMH model advocates, that their disagreements over the particulars of how, when, and where modern humans emerged entail disagreements over several basic issues in general evolutionary biology—e.g., those of anagenesis vs. cladogenesis, punctuated equilibrium vs. gradualism, and relative importance of gene flow, drift, and natural selection.

This situation has led Willermet and Clark (1995) to propose that the MRE-RAO dispute is a paradigm crisis, in Kuhn's (1970a) sense. In their view, each camp has its own assumptions and presuppositions about what data are relevant, what sorts of questions may be asked about the data, and how to ask them—in short, its own paradigm. The result is that the two camps select different crucial variables (which they simply assume to be evolutionarily meaningful), sample the fossil record with a bias toward the fossils most supportive of their views, see different "self-evident" patterns, and then accuse each other of missing the point. Since different paradigms are incommensurable, Willermet and Clark expect this impasse to continue until paleoanthropologists "confront the inferential basis for their knowledge claims" (1995:490).

In the rest of this paper we will examine issues raised by these critiques focusing on deficiencies, not in our database, but in the interpretation of it by MRE and RAO proponents. And we will consider whether a Kuhn-

ian paradigm crisis is preventing a satisfactory resolution of the AMH impasse.

THE COMPETING MODELS

The models under discussion can be viewed in historical perspective as the latest manifestations of two persistent opposing tendencies in the interpretation of human evolution (Spencer, 1984; Trinkaus and Shipman, 1992; Stringer, 1994; Smith, 1991; Smith and Paquette, 1989; Brace, 1981). These tendencies are manifested in competing views of human evolution as metaphorically either a stream or a bush. They originally emerged over the question of the evolutionary status of the Neanderthals, but have gradually widened to encompass human evolution generally.

MRE is the culmination of an intellectual lineage traceable back to Schwalbe and Manouvrier in the late 19th century, through such figures as Gorjanović-Kramberger, Hrdlička, Weidenreich, Weinert, Arambourg, Coon, and Brace. Despite their differences, these workers have interpreted human evolution since at least the emergence of *Homo erectus* as a gradual, continuous process (perhaps in discernible stages) marked by regional continuity and the unity of the human species. Neanderthals are viewed from this perspective as ancestral to later European populations.

RAO's intellectual forebears can be traced back to Boule and Keith, through Elliot Smith, Hooton, Vallois, Sergi, Howell, and Howells. Again despite considerable disagreement, these workers have seen important evolutionary divergences (if not necessarily speciations), discontinuities, migrations, and extinctions in the Middle and Upper Pleistocene human fossil record. They have accorded the Neanderthals (at least in Europe) little or no role in the ancestry of later Europeans.

It would be misleading to identify these tendencies as "schools." In each case, advances in paleoanthropology over the past century have radically changed the database and its interpretation. Major controversies have been laid to rest (e.g., Piltdown and Presapiens), the database has been vastly enhanced, and paleoanthropology has incorporated the Darwinian synthesis and

subsequent developments in evolutionary theory and molecular biology. Yet, through all these transformations, each tendency has persisted—waxing and waning in acceptance, changing to incorporate new data and theory, but never disappearing.

In some cases both tendencies can be found in the work of one individual. A prime example is Sir Arthur Keith (Keith, 1915, 1949; Trinkaus and Shipman, 1992; Spencer, 1979). Prior to Boule's study of the La Chapelle Neanderthal, Keith accepted a unilineal view of human evolution. In 1911–12, his viewpoint changed, and he began advocating an ancient origin for modern humans. This timing coincided as well with the "discovery" of the Piltdown remains. By 1915, Keith argued that Neanderthals were a distinct type who disappeared suddenly, being replaced by modern humans. At the time, Keith clearly believed his antiquity hypothesis to be the minority view (1915:209). Throughout most of his professional life, Keith promoted an ancient origin of modern humans, but in 1949 he published an essay proposing a view similar to Weidenreich's. (In that essay he dates his change of mind to 1931.) He still thought "Caucasian invaders" replaced Neanderthals, but the invaders themselves were originally of Neanderthal origin farther east. Another example of change in outlook appears in the work of Schwalbe (Brace, 1964, 1981; Trinkaus and Shipman, 1992). Although associated with an early unilineal view, he, like Keith, was converted by Boule.

In addition, among scholars who do not display a major change in perspective, there is more complexity in their writings than a simple assignment to competing schools would indicate. Hrdlička, who argued for a Neanderthal Phase of Man (1927), preferred a bushy to a straight phylogenetic line. Further, he advocated a monocentrist position, with migration *from* Europe (Spencer and Smith, 1981; Spencer, 1979). Weidenreich's famous criss-cross diagram (1947) displaying regional evolution does not include the classic Western European Neanderthals, "[p]erhaps because he couldn't decide what to do with them" (Trinkaus and Shipman, 1992:274). Tabūn and Skhūl are placed below the Cro-Magnon group of Western

Europe. Weidenreich (1943:47) stated that “[t]here is no reasonable cause for excluding the European Neanderthals from the evolutionary line. Quite another question is, of course, as to whether or not the first representative of *Homo sapiens* in the Late Pleistocene of Europe was a descendant of the European Neanderthals.” Hooton (1946) advocated replacement, and yet allowed for some interbreeding between Neanderthals and moderns. In the opinion of a geneticist writing at the time (Dobzhansky, 1944:265) “[i]f the hominid evolution took place within a single species subdivided into races, the differences between the classic theories of evolutionary divergence and Weidenreich’s theory of parallel evolution of hominid stems do not appear very serious.”

Nonetheless, whether the differences are serious or not, the most popular view has fluctuated over time. Once Dubois’ “*Pithecanthropus*” entered the fossil debate, Neanderthals looked less beastly and more suitable as ancestors (Trinkaus and Shipman, 1992). After Boule’s monograph, Neanderthals as direct ancestors seemed less likely. By the mid-1920s, Spencer (1979:569) reports, Hrdlička found himself “almost alone in placing the Neanderthals in the mainstream of human evolution.” At the time of his 1927 lecture, the *Presapiens* view was accepted by the majority of workers; Elliot Smith’s Pre-Neanderthal theory provided an alternative for those who preferred a later branching of Neanderthals from the modern stem. The former remained most popular until after World War II (Spencer and Smith, 1981). Subsequently, F. Clark Howell’s incorporation of the ideas of the evolutionary synthesis in his papers (1952, 1957) strengthened the Pre-Neanderthal hypothesis.

The current situation began to emerge in the mid-1980s, after a period characterized primarily by continuity thinking. Straus and Cave (1957), among others, pointed out the errors in Boule’s reconstruction of La Chapelle; Brace (1964) chastised his colleagues for their “hominid catastrophism”; and Solecki’s excavations uncovered flower pollen at Shanidar (Trinkaus and Shipman, 1992). Brace’s influential adaptationist perspective (1964, 1979) linked the morphologi-

cal changes across the archaic-AMH transition to changing selective pressures associated with important shifts in human adaptive behavior. Thus, heavy browridges and other facial architecture were seen as having gradually reduced in response to changes in behavior, such as less use of teeth as food processing tools and use of improved hearths for cooking. Following his lead, Brose and Wolpoff (1971) argued for continuity between Middle Paleolithic and Upper Paleolithic tool industries as well as between Neanderthals and AMH populations.

What we might call discontinuity thinking saw a resurgence in the 1980s, in part because of a series of detailed empirical studies whose conclusions emphasized the distinctiveness of the Neanderthals both from their contemporaries elsewhere and from subsequent Europeans (e.g., Stringer, 1974, 1982; Trinkaus, 1976, 1977, 1983; Trinkaus and Howells, 1979). Workers were struck by just how different Neanderthals were from later people when examined in unprecedented detail. They were led to ask whether they really could be ancestral to them; for some, the answer that emerged was “no.” Another factor was arguably the impact of theoretical developments in evolutionary biology, notably punctuated equilibrium theory (Gould and Eldredge, 1977, 1993), which entailed extinctions, migrations, and rapid evolutionary change.

A series of important conferences in the 1980s helped to increase the salience of the AMH issue and crystallize the positions of emerging MRE and RAO camps. An unprecedented level of attention to paleoanthropology in the popular press (including a 1987 *Newsweek* cover depicting “African Eve”), with much emphasis on the Wolpoff-Stringer conflict, no doubt helped to exacerbate the conflict. Indeed, the dichotomization in this controversy has been striking. Workers whose positions could be seen as intermediate between the RAO and MRE extremes, such as Bräuer (1989, 1992) and Smith (Smith and Paquette, 1989; Smith, 1992), have been drawn toward one pole or the other of the conflict rather than toward common ground.

The Recent African Origin model

The foremost proponent of RAO, Christopher Stringer, has presented this model in a series of publications (see especially Stringer and Andrews, 1988a; Stringer, 1989a, 1989b, 1992, 1993, 1994, 1995; Stringer and Gamble, 1993; Stringer and Bräuer, 1994; Stringer and McKie, 1996). Stringer's 1994 chapter provides a thoughtful chronicle of the development of his thinking on AMH origins.

Stringer does not shrink from the necessity of testing competing models. Indeed, he and Andrews (1988a) contend that one advantage of RAO and MRE over intermediate positions is their readier testability, owing to their more extreme predictions. In several publications (Stringer and Andrews, 1988a; Stringer, 1989b, 1993), contrasting predictions (or retrodictions) of the competing models are detailed and compared to various datasets. For instance, he predicts (Stringer, 1989b) that, under RAO, early AMH fossil samples around the world should show few differences among themselves (having had little time to accumulate them since radiation from Africa). Furthermore, as we move back in time, progressively older AMH should be progressively more similar to each other. The sets of predictions in these papers are not identical, though they are mutually consistent. Stringer finds RAO consistently preferable to MRE in these tests, though he and Andrews admit that the Australasian record poses problems for RAO as well as MRE. One observer, Wood (1994), judges the test involving Penrose shape distances among modern and fossil samples in Stringer (1993) to be more convincing in casting doubt on MRE than in confirming RAO.

Stringer's work, and the RAO model, are heavily skull- and face-oriented. These skeletal elements are both informative and over-represented in the fossil record; but as Wood (1994) points out, if mosaic evolution has occurred in the emergence of AMH, other parts of the skeleton might show different patterns of variability in time and space than do skulls, and any model must eventually accommodate all skeletal data.

Stringer's usual procedure involves statistically comparing various characteristics of

craniofacial size and, especially, shape (mainly measurements, indices, and angles). He compares fossil and recent samples from various regions of the world, and concludes that early AMH fossils worldwide resemble each other far more closely than they do their regional archaic predecessors. To Stringer, this makes sense only if the early AMH specimens are closely related through recent common descent to each other, and not to regional archaics. He also details problems of poor dating and fragmentary remains of fossils crucial to the MRE case in East Asia, Australasia, and Central Europe (e.g., Stringer, 1989a). He rejects MRE claims that many traits demonstrate regional continuity (e.g., flattened frontals and heavy brows in Australasia) on grounds that they fail to recognize plesiomorphies (shared primitive retentions) derived from distant common ancestors. His goal of reconstructing an ancestral morphotype of AMH (Stringer, 1992), retrodicted from the morphology of known early modern specimens, illustrates his concern with analysis of overall, even abstracted patterns of form in the fossil record—a very different emphasis from Wolpoff's, as we shall see.

Stringer (1994) has discussed his shift over time from a phenetic approach to a cladistic one. For him, the emergence of AMH was an evolutionary event that occurred in Africa (though Southwest Asia cannot be ruled out). A new hominid species arose, eventually spreading out to displace the species already occupying other regions. If any interbreeding took place between this species, *Homo sapiens*, and archaic indigenes, it was evolutionarily insignificant. Stringer shows some irritation at what he sees as irrelevant or biased criticism of this conclusion from the MRE side. For example, he complains of charges that he asserts complete genetic isolation between the modern and archaic human species, and envisions a "Pleistocene holocaust" perpetrated by "killer Africans." He stresses that he has never ruled out interbreeding between moderns and archaics. His evolutionary definition of species (Stringer, 1992), in the tradition of Simpson (1961), allows for interbreeding between closely related species if they maintain their distinct identities over

time. He notes that speciation, migration, and replacement are accepted as common processes in vertebrate evolution, and do not require violent extermination.

Stringer also rejects complaints about his failure to supply an explanation for the evolutionary success of AMH. Why, it is asked, were early moderns apparently confined to Africa and Southwest Asia for many millennia after their emergence? And what selective advantages allowed them later to spread rapidly and displace indigenous archaic populations, who were presumably long- and well-adapted to their environments? After suggesting (1989a) that the development of Upper Paleolithic technology may have been necessary to supply AMH with a competitive edge, Stringer (1994) later responded simply that he has made the case that the replacement occurred; the question of how it did so is an important but separate issue. After all, no one disputes the evolution of bipedalism from quadrupedalism among proto-hominids simply because we still lack a fully satisfactory evolutionary explanation for it.

However, in a recent popular book, Stringer and McKie (1996) sketch a more detailed response to this criticism. First, they throw the implausibility charge back at the MRE camp, attacking multiregionalism as requiring improbably high levels of gene flow among tiny, isolated human populations in drastically differing environments over hundreds of millennia. Stringer and Bräuer (1994) had already responded to repeated depictions of RAO as extreme by turning tables and attacking MRE as the extreme position. Indeed, Stringer and McKie charge, MRE is inconsistent with what we know about mammalian evolution generally, which abounds in cases of punctuated equilibria, species formation in isolated populations, migration, replacement, and extinctions. Why should humans be different? They even imply that a Kuhnian paradigm shift is underway: "We are witnessing a rare moment in science, the replacement of a redundant orthodoxy by a formerly heretical vision" (Stringer and McKie, 1996:140). They stress, however, that RAO is "heretical" only within paleoanthropology; in evolu-

tionary biology generally, it is unexceptionable.

As detailed above, the most popular view of human origins has varied over time. Thus what is heretical and what orthodox fluctuates, depending on place and time. Stringer may feel like a heretic in some quarters, but his basic perspective on modern human origins is an orthodox one among 20th century paleoanthropologists such as Howells (1944, 1993) and Howell (1952, 1957). Stringer and Gamble (1993:34) trace the current regionalist perspective back to Brace's 1964 paper, when "the pendulum swung back in favour of earlier workers such as de Mortillet and Hrdlička, as well as Weidenreich." It is open to debate how far the pendulum really swung, and it is equally possible to argue that the regional view represents rebellion against the orthodox.

Stringer and McKie (1996) then propose a more detailed scenario of AMH origins and spread than Stringer has heretofore presented. After eschewing much reliance on DNA work in recent years, Stringer here returns to citing recent research on both mitochondrial and nuclear DNA (e.g., Horai et al., 1995; Cavalli-Sforza et al., 1994) as independently confirming RAO. Next, he notes that recent work in comparative linguistics has generated language phylogenies remarkably similar to population phylogenies inferred from DNA (Cavalli-Sforza et al., 1988; Penney et al., 1993)—another independent line of evidence indicating a recent, African origin for all humans.

Stringer and McKie then cite DNA work suggesting one or more major bottlenecks in AMH population history between 150,000 and 50,000 years ago (e.g., Sherry et al., 1994; Relethford and Harpending, 1995; but see Ayala, 1995, for a dissenting view). In this view, modern populations finally emerged from this bottleneck and quickly spread around the world, starting perhaps 50,000 years ago. Stringer and McKie explain this success, like Klein (1995), by suggesting the evolutionary emergence of the capacity for complex language long after the appearance of modern anatomy. This capacity also would explain the archaeological transition to the Upper Paleolithic and its accompanying "creative explosion." While

this popular account cannot present Stringer's case in detail, it indicates his thinking on how the successful spread of AMH happened.

The Multiregional Evolution model

The second main model in this debate, MRE, is considered by Stringer to be an extreme view in polar opposition to RAO. Wolpoff, however, consistently argues that MRE is a moderate position. For example, he (Wolpoff et al., 1994a) purports MRE to be the "middle ground" between RAO views on the one hand and other possible regional views such as "stages" of evolution (Arambourg, 1958; Brace, 1995a) and Coon's (1962) regional evolution on the other. RAO views are cast as truly extreme, and the likely implication of different species with limited or no gene flow is stressed (Wolpoff, 1989a, 1989b, 1992; Frayer et al., 1993).

Thus characterized, MRE is reasonable and RAO is unlikely, given what we know from the fossil hominid and archaeological records. For example, consider the claim that "[s]ince the Eve theory requires *total* replacement in *all* areas, the demonstration of continuity in *just one* area would be enough to show that it is false . . ." (Frayer et al., 1993:41, emphasis added). Since complete replacement *is* an extreme view, MRE appears to be the moderate alternative. However, other claims in support of MRE are far from moderate. To select only one example, one closely following the statement quoted above:

We find neither specimens nor traits that could reflect an infusion of *any* African genes and their so-called more-modern morphology. There is an absence of features in the earliest moderns of *any* region outside Africa that can be *uniquely* traced back to Africa [Frayer et al., 1993:41, emphasis added].

The first sentence is a bold proposition, but even here the following sentence leads us back toward the "middle ground." To accept MRE, we have only to discard a *uniquely* African source.

In its details, MRE is a complex, intricate evolutionary model. Wolpoff thinks this model can explain three patterns he detects in the Pleistocene fossil hominid record: an initial contrast in characters and character variation between the geographic center and

geographic "edges"; the earlier appearance of regional continuity features at these edges; and the maintenance through time of these resulting regional contrasts (Wolpoff, the Center and Edge hypothesis, 1989b; see Mayr, 1963, Chapter 13 for a description of "central and peripheral populations" within a species). Gene flow is balanced against selection and/or drift in an isolation-by-distance model derived from Sewall Wright. Waveform analogies are employed to suggest a continuous fluid network that nevertheless can manifest individual local properties.

The articulation of the Center and Edge hypothesis within a broader evolutionary framework is critical for MRE. Wolpoff (1989b) claims his model *is* the gradualist model, but other evolutionary theorists who espouse gradualism and stress clinal variation in the context of regional evolution may nevertheless differ with regard to evolutionary mechanisms (Livingstone, 1992, 1996). In Wolpoff's view, his model is consistently misunderstood, particularly the role of gene flow within it. The persistent critique is that gene flow cannot produce coherence among populations of a species spread across large portions of the Old World, as Middle and Late Pleistocene populations were. Yet to Wolpoff, this is a non-issue: "The idea that gene flow magnitudes can be too small or too large makes no sense in terms of the *balance* model of clines" (Wolpoff, 1989b:89, emphasis added). One critic describes MRE as stretching "the bounds of credulity" and complains that "[t]here is serious need for normal procedures in evolutionary biology to prevail" (Howell, 1994:306). If Wolpoff is to succeed with his Center and Edge hypothesis of MRE, he will either have to provide a more convincing link to the wider arenas of population genetics and evolutionary theory as they apply in other animals, or provide a convincing case that hominids are, to a large degree, exceptional in their evolutionary development (e.g., through culture—see below).

Within a Center and Edge hypothesis of MRE, the supporting evidence must come in the form of specific regional traits, characteristics which vary dependably across geographic space. If all the hominids being

discussed are members of a single species of interconnected populations, the genetic bases for these traits will be allele polymorphisms and not, primarily, divergent alleles. Wolpoff (1989b:88, emphasis added; original in italics) states that "gene flow must primarily be regarded to function in changing *frequencies* of existing alleles, and not usually in introducing new ones." Again, the precise manner of genetic change and continuity needs to be detailed and parallels with other animals given (e.g., as with canids, briefly mentioned in 1989b), but the concept of varying frequencies is critical. It is the exact *combinations* of characters, not the presence or absence of traits taken independently, that allows us to distinguish regional lineages (Wolpoff, 1989b), proceeding in a similar manner to forensic ("racial") classification by means of modern skeletal material (Fraye et al., 1993). The gestalt, or total morphological pattern, delineates regional populations. The choice of regional traits or combinations thereof hence becomes critical. While both sides agree this should be done with care, they disagree about the choices (Stringer, 1992; Frayer et al., 1993).

Another prominent disagreement concerns transitional samples. How many examples of transitional specimens, which we hope represent (if imperfectly) populations in transition in the past, must there be for MRE to be supported or RAO refuted? What morphological features should we expect to find in each region? While Wolpoff proposes numerous transitional samples (e.g., Wolpoff, 1989a), he claims they need not be common (Wolpoff et al., 1988). Although he may be accused of inconsistency here (Stringer and Andrews, 1988b), recall that for Wolpoff one "refuting" instance would be sufficient. Furthermore, it is the *patterning* of changes that demonstrates continuity. Examples used include different patterns of tooth size reduction (Wolpoff, 1992; see also Brace, 1979) and a pattern of browridge reduction in Central Europe (exemplified by sequential samples from Krapina, Vindija, and Velika Pečina) that differs from the form reduction takes elsewhere (Fraye et al., 1993). However, a major difficulty arises in deciding whether transitional features or

fossils are due to in situ change or the effects of hybridization (see below).

Three final areas of controversy are the claim that modern human features emerge in widespread fashion; the role within MRE of the diffusion of ideas along with or rather than the spread of peoples; and the associated role of cultural behavior and the resulting uniquely hominid evolutionary pattern.

First, it is claimed that "anatomically modern" *Homo sapiens* cannot be acceptably defined (Wolpoff and Caspari, 1996), and more controversially, that *Homo sapiens* is not distinct from *Homo erectus* (Wolpoff et al., 1994b; Wolpoff, 1996). We will return to these claims, but for now the important point is the implication that the evolution of modern features occurs within one widespread but interconnected species. MRE proposes a sort of equal-opportunity modernity; no one region holds a monopoly on the evolution of modern traits. In essence there is no definable origin because the multitude of different characteristics we associate with modern human form pop up at different times and places across the Old World. For example, the midfacial region looks modern early in China, but Tabūn B has a chin, and modern brows occur at Klasies River Mouth (Fraye et al., 1993). Thus the origin of modern humans is not a biological "event," happening in one region at a given time.

Furthermore, Wolpoff has suggested that the diffusion of ideas along with that of people together produced the Upper Paleolithic populations of Western Europe. Modern humans, Wolpoff has said, "are a state of mind" (1996:629). This suggestion has parallels with both Klein's (1989) suggestion of neurological change co-occurring with the origins of behaviorally modern humans, and with the suggestion of Stringer and Bräuer (1994) that modern humans may have had a broader behavioral, rather than strictly technological, advantage over non-moderns, with the critical difference that Wolpoff is proposing a series of interacting multiregional neurological and cultural developments. Modernity is *not* "a package of very successful, interacting anatomical features and behaviors, whose genesis is at its origin" (Wolpoff and Caspari, 1996:169). It would better resemble a patchwork quilt sewn from pieces

collected from various locales at different times.

If we cannot put the critical behavioral changes together in one package with the morphological ones, and if the former changes are nevertheless crucial in the emergence of modern humanity, then we may be left with a model of our own evolution that is unparalleled among other animals. One could argue that the single-species hypothesis has not died. Rather, it has temporally repositioned itself. Culture may not provide a sufficiently sharply narrowed niche for all hominids, but it does provide such a niche for *Homo*, leaving aside the troublesome habilines. Wolpoff does not emphasize this point, but the underlying current is there, and one can argue that it is necessary for his model. Wolpoff et al. (1988:241) state that

[t]he multiregional model does not suggest that recent variation is solely the product of middle Pleistocene ancestry. This would ignore the fundamental role of local environmental selection in accounting for human variation accepted by all models and deny the possibility that the spread of culture may provide selection for common evolutionary directions without gene flow.

What else besides culture would provide a sufficiently strong selective force to tie hominid populations across the Old World together as one species? And if culture does indeed provide this role, how is MRE to be distinguished from the "stages" model that Wolpoff rejects?

Further, since culture is considered a uniquely human adaptation, we encounter the problem highlighted by Cartmill (1990) of providing a scientific explanation for a uniquely human quality. Although by definition humans alone have a cultural adaptation, parallels with the adaptive behavior of other animals need to be presented in order for the model to retain theoretical coherence. Foley (1987) has rejected "culture," as an integrated concept, in explanations of human origins, advocating instead reduction to the behavioral components of culture. Through this method, he thinks, we may better achieve an appreciation of the behavioral continuity between humans and other animals.

In one way or another, however, we must involve a consideration of culture in human evolution if we are to retain ties not only to

evolutionary biology but also to archaeology and to anthropology more generally. Brace has persistently argued (1964, 1979, 1995a, 1995b) that the primary means through which hominids adapt is culture. Using the principle of competitive exclusion, culture was argued (Brace, 1964) to be the hominid adaptive niche, thus providing an ecological rationale for the single species hypothesis, which he still endorses for the later periods of human evolution. Following Weidenreich (1940, 1943, 1946, 1947; see also Dobzhansky, 1944), Brace envisions gene flow as sufficient to maintain populations across the Old World as a single-species, *provided that* we recognize the powerful role of selection acting via a cultural adaptation.

Templeton (1993:69) argues for "restricted gene flow among Old World human populations with no single source population for all genetic variation." However, Livingstone (1992) thinks MRE advocates have relied too heavily on the role of gene flow as a unifying factor within species. While selectively advantageous genes will spread, his simulations show that beneficial mutations will often arise independently in distinct populations before one such mutation can spread among a large network of populations. A focus on adaptation and the selective forces induced by culture might help to provide a resolution in the modern human origins debate by clarifying the respective roles of natural selection and gene flow in human evolution.

Brace has recently complained (1995b: 711) that there is an "absence of a concern for evolutionary dynamics" and that "the concept of adaptation has almost entirely disappeared from consideration." He distinguishes his view from MRE by stating that the cultural focus of his regional view has been retained merely as an indication of gene flow:

the "multiregional" continuity view . . . has made no effort to explain why the new aspects of form emerged where they did or what advantages they conveyed that should have led them to prevail in the areas to which they are supposed to have spread by means of gene flow . . . [Brace, 1995b:713].

In 1979, Brace presented his "culinary revolution" hypothesis to explain reductions in

tooth size from the Krapina Neanderthals to their successors. Although there is no consensus on this point at present, Brace would argue that there are behavioral shifts documented in the archaeological record that are correlated with and help to explain the appearance of modern human form. To his tooth reduction evidence can be added the MSA bone points discovered by Brooks and Yellen in Zaire (Brooks et al., 1995; Yellen et al., 1995); Shea's (1993) microwear analyses of Levantine Mousterian tools which suggest hafting of spear points by Neanderthals as well as early modern humans; and Lieberman's and Shea's (1994) analysis suggesting that the foraging and mobility patterns of Levantine archaics were different, but not less "advanced," than those of early moderns.

However much he might disagree with Brace on particulars, Trinkaus has also sought to discover Neanderthal adaptive patterns, showing less concern with Neanderthal phylogeny (Trinkaus, 1976, 1977, 1983; Trinkaus and Shipman, 1992). Brace's challenge to MRE from within the regionalist ranks and Trinkaus' neutral phylogenetic stance remind us that alternate routes around the AMH impasse may be available.

Expectations and points of disagreement

Having surveyed MRE and RAO, we can now turn to a consideration of the factors contributing to continued disagreement. Beyond the need for more and better data, several theoretical quandaries remain. It is unclear what both sides consider to be the appropriate or relevant data, the proper analyses to use, and even the sorts of questions that are fundamental to address.

What patterns of change are expected under each model? Must the patterning of variation, or the relationships of similarities and differences across populations, be consistent throughout the middle and early portions of the Late Pleistocene if MRE is the better model, as Stringer (1992) suggests? MRE theorists could accept Stringer's cladograms of genetic affinity between populations without thereby accepting his conclusions. For example, it could be maintained that the present genetic situation has been complicated by numerous recent migrations and is not easily transposed back beyond the

Neolithic (Wolpoff, 1989b). Given this difficulty, are there patterns of change through time that offer us a clear choice between these competing models?

Related to this question, can we distinguish in the fossil record the patterns of change expected with an in situ regional transition to AMH from those expected following migration from one region to another with significant hybridization between the migrants and indigenes? Bräuer (1992) has suggested that early modern Africans would be generalized in their morphology. While Stringer and Andrews' (1988a:1264) Table 1 lists "Modern regional characters of low antiquity at peripheries (except Africa)," Stringer and Bräuer (1994:417) contest Frayer et al.'s (1993) claim that in RAO the earliest modern humans should look "African," stating that "[t]he earliest modern humans should resemble their proposed African *ancestors* . . . and not *modern* Africans, except where it can be shown that modern Africans have also retained such ancestral features." Under RAO, modern regional features are predicted to have low antiquity.

Wolpoff (1992:56) states that RAO has one "unambiguous prediction": "Africans or African features should be identifiable in the earliest modern populations outside of Africa, unless the model has no predictions at all." Wolpoff in effect sets up for RAO what he believes to be an impossible task, namely documenting uniquely African features. Even if he is thinking of combinations of characters here, he does not think such combinations appear early in time in Africa. They would not be expected to appear early under MRE, since Africa is a "central" geographic region.

Exactly what would "more African" look like? Both camps agree that there should be regional (clade) features in Africa, and agree, for different reasons, that it will be hard to establish such links or features early in time. But if we cannot do so, and if present-day Africans are not an acceptable model, how can we decide whether those outside of Africa are "African-like"? With the Center and Edge hypothesis, we would expect central populations to be relatively undifferentiated, so how could we detect African influence elsewhere? How could we tell how

much gene flow occurred between relatively undifferentiated migrants and indigenous peoples elsewhere? How could we distinguish substantial gene flow from regional continuity with relatively little gene flow?

One possible solution is to focus on patterns of change to modern morphology through time that vary from region to region. For example, the pattern of browridge reduction at Vindija is said to be different from that seen elsewhere (Fruyer et al., 1993). However, this could still be the case even with considerable hybridization. Given the limitations of the fossil record, can we accurately discern varying levels of gene flow?

Another issue concerns character selection, definition, and polarity. Stringer (1992) has stressed careful definition and review of proposed regional characters. What is the evolutionary significance of the variables used in these analyses? The genetic and developmental bases for many of these traits remains largely unknown. However, some recent work along these lines has begun.

Caspari (1991, cited in Fruyer, 1992) has examined the morphology of the posterior cranial vault, concluding that the supra-orbital fossa, common among Neanderthals, is a resorptive surface; its loss among post-Neanderthal populations may be due to functional changes in nuchal musculature. Crummett (1994, cited in Wolpoff, 1996, and Lieberman, 1995) has analyzed shovel-shaped incisor form and divided shoveling into three components: marginal ridges occurring on the lingual surface, enamel tubercles at the base of the lingual surface of the tooth, and degree of labial convexity (curvature). These three components combine in different ways in different regions.

Lahr (1994; see also 1996) selected 30 regional traits and reported that only 11 showed higher frequencies or significantly different mean values in the regions predicted by MRE. Further, she suggests that some cranial superstructures which *do* show strong regional distributions are plesiomorphic ("primitive") and therefore of low phylogenetic value. Third, comparing samples from Afalou and Taforalt with more recent Africans leads her to suggest that regional

traits may sometimes be of restricted temporal depth.

Lieberman (1995), choosing 33 craniodental and mandibular characters, finds more support for RAO than MRE, but cautions that three criteria that should be met by proposed traits often are not. To be useful in phylogenetic analyses, traits should be developmentally homologous; they should be shared derived characters (synapomorphies); and data for the characters should be available for both fossil and living samples from all regions.

While Wolpoff (1996) uses Crummett's incisor shoveling research to support MRE, Lieberman (1995) thinks it does not provide support for MRE because the characters are not synapomorphies or are too variable. On the other hand, Wolpoff (1996:785) thinks "Lahr's study of regionality is flawed by its lack of focus on a clear refutatory procedure and her choice of a test criterion—she disregards the critical observation that past and present regionality is marked by *differing combinations* of features, and not particularly by differences between their individual frequencies." (He also complains about the heterogeneous nature of her samples.) Yet Lieberman (1995:166) states that "[t]he argument that regional human clades are best characterized by *combinations* of derived and primitive characters rather than by any *specific* derived characters is illogical." Thus there is continuing disagreement over not only what the predictions of the models should be, but how to test them as well. What one side considers a test, the other considers an analysis riddled with difficulties.

A further problem found by Willermet (1993) is that different samples, and sets of traits from them, are used in attempting to support or test opposing models. She collected published cranial measurements for *Homo erectus* and archaic and modern *Homo sapiens*. (It should be noted that her sources of measurements include publications of Stringer but not of Wolpoff.) Only 11% of the database surveyed (cranial measurements on individual specimens) was employed by both MRE and replacement (RAO or "Out of Africa," "Out of Asia," and "Afro-European *sapiens*") advocates. There was not enough

common ground for her to test predictions of these models statistically.

In addition to sample composition differences and selection of different traits to measure or categorize, different approaches to data analysis are evident. Stringer (e.g., 1989b, 1993) relies more heavily on multivariate cranial analyses while Wolpoff tends to concentrate on specific morphological features or complexes. Wolpoff (1989b:76) complains that there has not been "a discussion or refutation of the detailed morphological data presented by the fossil evidence which *supports* the regional evolution hypothesis." This complaint suggests that Wolpoff expects a point-by-point refutation of his morphological descriptions of the fossils and the trends based on them. He wants Stringer to play his game, but Stringer may be playing a different game altogether. From Stringer's point of view (Stringer and Bräuer, 1994: 416), in the MRE camp "[n]o attempt is made to test hypotheses of relationship, since these are assumed to be there and are merely to be supported by data collection." Elsewhere Stringer (1989a:72) tells us, regarding claimed Neanderthal-AMH morphological continuities in Central Europe, that "[a]pparent evolutionary trends . . . can be demonstrated, but they do not prove continuity." Exchanges such as these highlight fundamental differences in the approaches to the data. How else is one to build a case for continuity other than by searching for trends through time in specific details in regional geographic areas? A continuity perspective leads one to focus on regional sequences in vertical time depth. On the other hand, if one holds an underlying expectation of replacement, there will be a greater tendency to focus on sequential, "horizontal," time segments from across the Old World. Obviously both sides "see" both "views," but the focus of attention is different.¹

Another fundamental difference is suggested by Stringer (1992) regarding the reconstruction of an "ancestral morphotype"

as being an "essential task." Stringer wants to create an abstraction—put all the data together, and create a plausible ancestor for the known descendants. (One could, incidentally, do this through multivariate methods; in fact, without such methods it would probably become an artistic exercise.) Wolpoff would likely see this as abstracting away from reality. Another factor that enters here is an emphasis on individual variation. If one creates such a morphotype as Stringer suggests, will it not necessarily be more generalized than its descendants? And yet real ancestors will have had particular traits, and these need not have been more generalized *according to MRE*. Creating an abstraction will thus create, necessarily, a generalized form, and the latter will "prove" the generalized nature of our ancestors. But if we cannot abstract first and test later as Stringer suggests, how are we to proceed in an objective manner? Perhaps it would be better to study specific regional sequences, focusing on details in the context of total morphological patterns, while being fully aware of the large degree of individual variation; but is it reasonable to expect to do this accurately without complex methods of analysis? If combinations of features rather than individual details are of primary importance, would not multivariate methods be the techniques of choice, even with the admission that samples are limited and often fragmentary, and that thus some "creativity" in the use of these methods is required? Yet what is one to conclude when, to use a revealing quote from Frayer et al. (1994: 428), a multivariate analysis (by Stringer) "draws relationships that the eye can so easily discount"? (See also here the discussion of the Klasies zygomatic [p. 434]: "no amount of argument could negate what visual comparisons so clearly show. . . .")

In the realm of background expectations lies an "extra-scientific" factor that nevertheless is relevant. Individual experience with particular fossils may play a large role in convincing a researcher of certain points of view. Quoted in a popular book (Shreeve, 1995:85), Stringer says that when confronted with Jebel Irhoud for the first time, "I could see right away that this thing wasn't anything at all like a Neanderthal, at least

¹Shreeve (1995) employs a horizontal/vertical distinction between "stages" and MRE; the analogy here suggests a type of "horizontal" view applies to RAO. Both "stages" and RAO perspectives emphasize "horizontal" similarity, but interpret this as due to different causes. In the first case, selection through common adaptation and culture is stressed; in the latter, similarity is due to recent common ancestry.

not in the face." Wolpoff (Shreeve, 1995:102) recounts a striking experience of his own. He says that after reconstructing the Sangiran 17 face: "I nearly dropped dead. Instead of being just another *erectus*, here was this great big, hyper-robust Australian aborigine. I knew at that moment that Thorne was right, and I was wrong" (regarding the Center and Edge hypothesis of MRE). Lest one be left with the impression that these are isolated incidents, similar examples without relevance to the modern human origins debate can be given. Walker (Walker and Shipman, 1996) was convinced by his experience of reconstructing KNM-ER 1470 that it showed strong australopithecine affinities, a conclusion not popular with two of his co-workers (Richard Leakey and Bernard Wood). One of us (SLS) was impressed by an examination of casts of *A. afarensis* proximal pedal phalanges. After months of measuring modern human toe bones, one thing immediately obvious about these bones was that they were not like modern human toe bones. Background preparation and individual experiences can have large effects on interpretations, but this is not generally discussed in scientific articles, due to the subjectivity of the experiences, perceptions, and resulting convictions. These convictions may be based on a great deal of study and knowledge, but reasons accessible to others who have not had one's own particular experiences must be presented, for others may have had equally salient impressions incompatible with one's own.

THE BROADER CONTEXT

The debate over modern human origins can be placed in the context of several larger, unresolved controversies within evolutionary biology. Questions of the nature of evolutionary change and the process of speciation lie behind many of the narrower disagreements in this debate.

The proposal that evolutionary change occurs through punctuated equilibrium (Gould and Eldredge, 1977, 1993; Gould, 1980), rather than through the gradual accumulation of small changes over time, has affected all areas of evolutionary biology. The RAO model fits comfortably within a punctationalist framework whereas MRE

derives from traditional gradualist concepts of the modern synthesis.

A related development has been the rise of cladistic methodology (Hennig, 1966). While embracing punctuated equilibrium as a model for evolutionary change is not necessarily associated with enthusiasm for cladistics, there is a tendency for the two to go together. Related to the use of cladistics, in turn, is emphasis on cladogenic speciation and a de-emphasis, if not denial, of the role of anagenic change, particularly across species "boundaries." Another associated tendency is for those using cladistics to be taxonomic "splitters," in order to create convenient clade units for analysis. In contrast, an emphasis on the large degree of individual variation among modern taxa leads one toward a "lumping" approach to classification.

What views one holds on these broader issues affect choice of RAO vs. MRE models. A worker who operates with a punctationalist model of change, uses cladistic methodology, emphasizes cladogenic speciation, and is a taxonomic splitter is likely to lean toward RAO if not to leap enthusiastically on its bandwagon. An evolutionary gradualist who thinks that anagenic change is important, is wary of cladistics, and emphasizes individual variation (and thus prefers taxonomic lumping) is likely to be inclined toward some version of MRE.

These broader issues affect species concepts and definitions as well, and the confusion surrounding the nature of species and speciation has implications for the debate over AMH origins. Both Wolpoff and Stringer profess an evolutionary concept of species, but they probably neither envision nor apply this concept in the same fashion. They certainly arrive at different interpretations of relationships, and the model of evolution of polytypic species employed by Wolpoff (1989b) separates his concept of species change, at least for hominids, from that of Stringer.

Wolpoff (1996:57) defines an evolutionary species as "a single monophyletic lineage of ancestral-descendant populations evolving separately and maintaining its identity from other such lineages, with its own evolutionary tendencies and historical fate." The evo-

lutionary species, he claims, has both a genealogical basis and a genetic basis. That is, the concept includes aspects of both reproductive isolation (the genetic basis, shared with the biological species concept) and concern with monophyly and ancestor-descendant sequences (the genealogical basis, shared with the phylogenetic species concept). This overlapping quality allows Wolpoff (1996:58) to state, "when focus is on the dynamics of a species at a particular time, it is the biological species concept that applies."

Stringer (1992:19–20) quotes both Simpson and Wiley to argue that Neanderthals and early modern humans could have interbred and yet remained distinct species, without Neanderthals having "contributed significantly to the modern human gene pool beyond (possibly mobile) hybridisation zones." If such zones are admitted, perhaps there are similarities to the kinds of taxonomic quandaries presented by baboons in Ethiopia (Jolly, 1993), where there is a hybridization zone between *Papio cynocephalus* and *Papio hamadryas*. Furthermore, taxonomists disagree as to the number of species among savanna baboons. Like hominids, baboons are geographically dispersed and ecologically adaptable, making them a favorite comparative group for early hominids. Perhaps to some degree they provide a relevant comparison for later time frames as well.

The different concepts of hominid species evolution jump into sharp relief if we step back in time to the evolution of what has traditionally been labelled *Homo erectus*. Stringer (1994) splits "*Homo heidelbergensis*" off from *Homo erectus*. Wolpoff (Wolpoff et al., 1994b; Wolpoff, 1996) has endorsed a hyper-lumping scheme, subsuming *Homo erectus* into *Homo sapiens*. Admittedly there are messy boundary problems between late *Homo erectus* and early *Homo sapiens*. One could simply accept anagenic change across species "boundaries" and the resulting inevitability of taxonomic fuzziness during such periods. Nevertheless, lumping *Homo erectus* into *Homo sapiens* potentially allows Wolpoff to move closer to a cladistic view emphasizing cladogenic speciation and more

circumscribed taxonomic definitions of species. It sidesteps the perceived problem of pan-Old World species change from *Homo erectus* to *Homo sapiens* and moves him further from the "stages" view of hominid evolution (Arambourg, 1958; Brace, 1995a) that he has rejected.

Inter-species problems remain, however. There is a problem of confounding grade and clade, a problem that Wolpoff (1992) thinks is probably inevitable. The difficulty of disentangling these two has led to persistent debate over the issue of "primitive" traits. Proposed regional characters within an MRE model may be labelled primitive, hence uninformative, by RAO supporters. (Notice the connection to cladistic methodology; with other analytical methods, primitive characters might not be so readily dismissed.) Labelling a character as primitive is to dismiss its phylogenetic importance, for if a trait is a retention from earlier hominid ancestors in general, it does not help distinguish among later possible ancestral-descendant sequences. Think of large back teeth and big browridges. These can be labelled primitive, or plesiomorphous, and hence dismissed. Wolpoff may complain (e.g., 1992) that it is the *patterns* of reduction or change that are regionally distinctive, but RAO advocates have yet to be convinced by these arguments. If grade and clade are truly confounded, primitive and derived become relative terms, not only temporally but also geographically. For example, big browridges are "primitive" for hominids but display differential persistence in Australia relative to Asia. As Wolpoff and Caspari (1996) note, this implies that no worldwide definition can be constructed for modern humans, because different numbers or frequencies of "primitive" traits may be displayed by various modern populations.

Another dispute regarding labelling of characters has surrounded the concept of parallel evolution. Wolpoff et al. (1988) contest Stringer and Andrews' (1988a) characterization of MRE as employing parallel evolution to explain the similarities among crania from different regions. Since both sides reject a Coonian multiregional view,

why is the phrase "parallel evolution" used? Again, there is a connection to cladistic methodology. "Parallel evolution" is an acceptable phrasing in this context if regional lineages are seen as distinct. Wolpoff et al. (1988) think cladistic methodology is misapplied below the species level; Stringer and Andrews (1988b) disagree. The use of cladistics encourages the perception of distinct units which are compared, and terms traditionally used for comparisons between species are transferred to comparisons within species.

Finally, RAO advocates display a tendency to allow neutral genetic changes a greater role in evolution than do MRE defenders, a tendency associated with other choices on the issues discussed above. Stringer and Andrews (1988a:1263), nearing the conclusion of their description of MRE, state: "The appearance of *Homo sapiens* was thus primarily the result of a continuation of long-term trends in human evolution, and it has occurred mainly through the re-sorting of the same genetic material under the action of selection, rather than by the evolution and radiation of novel genetic material and morphologies." While this quote may seem unexceptionable, it reveals implicit punctualist and cladistic thinking. To a gradualist, re-sorting under the action of selection could produce novel outcomes, but one does not receive this impression from their characterization of MRE. Genetic research has shown that there is a relatively low degree of genetic variation among modern humans. How one interprets this is affected by one's views concerning the relative importance of selective and neutral changes in human evolution. If neutral change is primary, then low genetic variation implies a recent origin for modern humans; but if selection has played a major role, it may indicate instead a significant role for long-term polymorphisms spread throughout human populations during our evolutionary development.

PARADIGMS AND THE PHILOSOPHY OF SCIENCE

Quoting Harrold (1991) as well as Kuhn (1970a), Willermet and Clark (1995) propose

that modern human origins research is in a state of "paradigm crisis":

It has become evident that the two camps are operating with different underlying preconceptions about what causes pattern in the human biological record, and how pattern should be measured [1995:487].

We think that the slow progress to resolution of the debate can be attributed to differences in the *metaphysical* paradigms of modern human origins researchers that in turn result in a biased selection of specimens and/or variables used in the analysis [1995:488, emphasis added].

Are RAO and MRE supporters operating with different paradigms? Wolpoff (1996) claims to follow a Popperian approach to science, and further claims (e.g., Frayer et al., 1993) that his critics misunderstand or do not follow such an approach. Since the philosophy of science has become an issue in this debate, with both Kuhn and Popper being invoked, it is relevant to examine the writings of philosophers of science in more detail.

Let us begin with Kuhn and the concept of a paradigm.² Kuhn's (1970a) notion of a paradigm is sufficiently broad that one can argue either for or against paradigm clash in the case of RAO vs. MRE. As Wisdom comments, "the concept of a paradigm . . . is a nice idea; but it is not easy to say just what it means" (1974:832). Paradigms are defined by Kuhn (1970a:viii) as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners." A paradigm is "an accepted model or pattern" but "like an accepted judicial decision in the common law, it is an object for further articulation and specification under new or more stringent conditions" (1970a:23). Another use of paradigm occurs later: "paradigms provide scientists not only with a map but also with some of the directions essential for map-making. In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture" (1970a:109). Kuhn employs various concepts

²The overview of Kuhn given here relies on the 1970 edition of *The Structure of Scientific Revolutions* (1970a) and his comments in *Criticism and the Growth of Knowledge* (Lakatos and Musgrave, eds.) published in the same year (1970b, 1970c). Clearly, Kuhn's philosophical writings show development of concepts through time. For an authoritative account, see Hoyningen-Huene, 1993.

of “paradigm.” Masterman (1970) lists 21 different senses!

These different senses do overlap, showing nuances of meaning Kuhn employs in elaborating his concept. To simplify, Masterman boils these 21 senses down to three: metaphysical paradigms, sociological paradigms, and artefact or construct paradigms. Ruse (1989a) divides the paradigm concept into sociological, psychological, epistemological, and ontological aspects. Kuhn himself, in the postscript to the second (1970a:175) edition of his book, divides his paradigm concept in two:

On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.

The first sense Kuhn takes as sociological. The second, deeper sense refers to paradigms as “exemplary past achievements.” The first is discussed under the heading “Paradigms as the Constellation of Group Commitments.” Those employing paradigms in this sense share a “disciplinary matrix” consisting of symbolic generalizations, models, and values. (He considers shared models, or shared commitments to beliefs, to be what others refer to as metaphysical paradigms.) The second sense, discussed under “Paradigms as Shared Examples,” refers to “exemplars.” Students become scientists through “doing” their science, which requires learning how to solve certain kinds of problems and recognizing similarities and connections between problems. The “rules” are understood implicitly through the practice of one’s discipline.

Since it is paradigms that are shared and not necessarily “rules, assumptions and points of view” (Kuhn, 1970a:42), we can ask how much has to be shared in order for scientists to share the same paradigm. When are differences fundamental and crisis-invoking?

According to Kuhn, most scientists spend most of their time doing “normal science,” defined as “research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as sup-

plying the foundation for its further practice” (1970a:10). Normal science “restricts vision,” but such focus is essential for scientific progress. Normal scientists are “puzzle solvers,” with a puzzle, by definition, being a problem that has a solution. Transition between paradigms involves “the previous awareness of anomaly, the gradual and simultaneous emergence of both observational and conceptual recognition, and the consequent change of paradigm categories and procedures often accompanied by resistance” (1970a:62). In the ensuing crisis state, there will be a “proliferation of versions of a theory” (1970a:71); novel theories arise in response to crises, and the paradigm shift occurs relatively quickly in most cases. Kuhn does not think alternatives are commonly tossed about in normal science. However, in crisis states paradigms are compared. If normal science is unable to contain the crisis and if the problem or anomaly is not simply set aside because no good solution is available, a new paradigm may be chosen.

Kuhn has caused himself much grief with philosophers due to his descriptions of how the choice between paradigms is made. He states that “[l]ike the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life” (1970a:94). This characterization, his assigning a role to “persuasive argumentation,” his use of the term “incommensurable” in describing different paradigms, and his suggestion that extra-scientific criteria are involved in paradigm choice due to the role of value judgements leave him open to accusations of irrationalism and relativism. He contests the former and the latter in the strong sense of the term.³

³Some philosophers have defended Kuhn on this point. Wisdom makes “a distinction between the empirical concept [content] of a theory and a mesh of ontology that goes with it”; ontology is “embedded” within the theory and accompanied by a *Weltanschauung*, or “special way of looking upon the world” (1974:833–835). He suggests that if the choice among competing theories involves decisions about embedded ontology and/or worldview, then it is understandable that it appears less rational than discussions involving solely empirical matters. Brown (1977) goes further. Employing a parallel with ethics, he defends Kuhn against the charge of irrationalism by proposing that it is precisely in making such decisions as those between competing theories, when we have no “algorithm” to follow, that judgement, involving rational decision making, is necessary.

Kuhn separates himself from Popper⁴ by stating that "[c]ompetition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another" (1970a: 8). Kuhn doubts that falsifying experiences exist: "If any and every failure to fit were ground for theory rejection, all theories ought to be rejected at all times" (1970a:146). Falsification "might equally well be called verification" because a new paradigm triumphs at the same time that an old one meets its demise; we "ask which of two actual and competing theories fits the facts *better*" (1970a:147). Falsification, therefore, does not explain scientific progress, at least in its naive form; but Popper's writings are more complex than many would suppose. Popper himself (1974b) complains of a "Popper legend" that is an inadequate simplification of his ideas.

In attempting to present Popper's philosophy, Lakatos (1970)⁵ goes so far as to separate tendencies with the use of subscripts, such as Popper₁ and Popper₂. While Popper (1974b) contests Lakatos' interpretations, Lakatos is not the only philosopher to have seen different themes in Popper's work. Brown (1977) views Popper's falsificationism as transitional between logical empiricism and newer approaches to the philosophy of science, such as Kuhn's. As a bridge between these two philosophies of science, Popper's writings display two strains, one a stricter falsificationism and another a more

moderate form diverging more strongly from the logical empiricist tradition. In a point similar to one of Kuhn's (see above), Brown rejects falsificationism as sufficient, since falsification of one conjecture may require that we have previously corroborated another; and if scientists are obliged to adopt the same critical attitude toward potentially falsifying statements as toward their theories, then "the duty to attempt to refute the basic statement is equivalent to a duty to attempt to defend the theory" (1977:74).

Kuhn (1970b) thinks Popper's concept of science ignores periods of normal science, and Popper (1970) agrees that he has done so. But Popper is distressed by the specter of normal science. "Normal science" is not, or better stated, should not, be normal to science. He dislikes the idea of it, thinks it is dangerous for science, and believes it to be a recent phenomenon (Popper, 1974b). Popper may be reacting negatively to Kuhn's description of normal science as puzzle solving, which does not connote great achievement. But Kuhn (1970b:13) does think that "[c]hallenges and adjustments are a standard part of normal research in empirical science." What he stresses, in distinction to Popper, is that logical criteria *alone* are incapable of providing a clear choice between competing paradigms (Kuhn, 1970b). Kuhn (1970c) presents Popper as placing more emphasis on purposefully seeking weak points in a theory; a scientist should be a critic at all times, and it is desirable to have alternative theories among which to choose. Kuhn argues instead that progress is better served through saving such critical behavior for selected occasions, while during "normal science" scientists focus more intently on the articulation of one theory.

Most examples used by both Kuhn and Popper are from physics and chemistry. Relatively little attention is given to evolutionary biology, and neither is now available for comment concerning the application of his views to disputes within paleoanthropology.⁶

⁴In *The Logic of Scientific Discovery* (1959) Popper is concerned with the basis for our knowledge claims, and his approach emphasizes the logical asymmetry between verification and falsification of universal statements. While we can never be certain that universal statements are true, because we cannot test all possible instances, one contradiction is enough, *logically*, to falsify a proposed statement or hypothesis. His basic problem, traced back to Kant, is one of demarcation between empirical science and metaphysics, and he proposes falsifiability for this criterion. There are degrees of falsifiability, the empirical content of a theory or hypothesis increases along with degree of falsifiability, and simplicity and degree of falsifiability can be equated. Theories are not verified, but they can be corroborated, and the degree of corroboration stands in inverse relation to logical probability. That is, the less likely an hypothesis is as initially proposed, the higher the degree of corroboration if it is not eliminated by testing.

⁵Lakatos himself adopts a version of methodological falsification, arguing for competing research programs rather than a concept of normal science. It is not the case that crucial experiments can overturn a theory, for scientists will save it through ad hoc adjustments. Criticism of long duration comes from competing research programs, and only if such criticism is constructive is success achieved.

⁶Popper does discuss Darwinian theory, and the development of his thinking regarding it is interesting. In 1974a (section 37 of the autobiography), he refers to Darwinism as a metaphysical research program. Rather than a scientific theory that is testable, it provides a possible framework upon which to build such theories. It is further an application of a form of logic Popper

Nevertheless, by this point it should be clear that both Kuhn's and Popper's philosophical ideas are complex and that no simple answer is forthcoming on the question of whether MRE and RAO share a common paradigm. However, we do not think that the modern human origins debate is the result of a lack of commitment to hypothesis testing by either side. Stringer and comrades are less Popperian only if one is referring by that appellation to strict falsificationism, dogmatic falsificationism, the "Popper legend," or whatever phrasing along these lines one wishes to adopt. Both sides list testable implications of their models. In some instances they agree on these implications; e.g., both sides agree that if RAO is the better model, then the earliest AMH should occur in Africa and early moderns elsewhere should resemble their African ancestors.

Clearly MRE and RAO are not competing paradigms in the sense of Newtonian mechanics vs. Einsteinian relativity or Ptolemaic vs. Copernican astronomy. Both MRE and RAO share a broad commitment to Darwinian evolutionary theory. A better case could be made for a paradigm shift within geology in the 1960s and 1970s. In geology, the acceptance of plate tectonics created a new framework of interpretation. Yet as profound a conceptual change as this was, Ruse (1989a) argues that it does not quite match a Kuhnian model of scientific revolution. Methodological considerations of what constituted good geoscience remained essentially the same. Further, Ruse posits that in addition to old facts being reinterpreted, *new* facts were discovered that were previously unavailable to scientists operating without a tectonic model.

refers to as situational; that is, given certain real-world conditions, it becomes "almost logically necessary" (1974a:134). By 1977, Popper had reconsidered his position on Darwinian theory and recanted. Though evolution by means of natural selection "is difficult to test" and "really severe tests of the theory of natural selection are hard to come by, much more so than tests of otherwise comparable theories in physics or chemistry" (1985: 242), it is testable. Further, his discussion of natural selection indicates how far Popper can be from rejecting a theory based on a few "falsifying" instances. Even in his earlier writing, where Darwinism is viewed as untestable, he nevertheless thinks it has great scientific value; it is relegated to a metaphysical program due to its lack of testability. It simply does not pass the demarcation criterion, however useful it may be in an heuristic sense.

To select an example within evolutionary biology, one might suggest an attempted paradigm shift in the challenge to the modern synthetic view made by punctuated equilibrium. Yet one could also argue that punctuated equilibrium has been successfully absorbed within the traditional synthetic theory and thus a crisis averted, suppressed, or overcome (see, e.g., Charlesworth et al., 1982). Ruse proposes that circa 1980, Gould thought about evolutionary processes "through a lens or filter of discontinuity" (1989b:122). Ruse suggests historical links between punctuated equilibrium and the continental European tradition of transcendental idealism, with its emphasis on form or structure (*Baupläne*), on the one hand, and Darwinian gradualism and the British philosophical tradition that emphasized function and adaptation, on the other. (For a recent discussion of Neo-Darwinist vs. Epigeneticist perspectives' effect on the modern human origins debate, see Churchill, 1996.) Ruse is quick to point out that these are tendencies of thought rather than dichotomous types, with some scholars emphasizing one over the other. He hedges a bit in addressing the question of whether punctuated equilibrium represents a paradigm difference with respect to traditional Darwinism. His qualifications to accepting paradigm status center around the less-than-gestalt contrast between the two, the historical depth of both views, and the possibility that both may persist as competing traditions.

In paleoanthropology, a better candidate for a real paradigm clash than the AMH issue is exemplified by the Bordes-Binford dispute over the interpretation of Mousterian lithic variability (Harrold, 1991). From the 1960s, American archaeologists, anthropologically trained and processually oriented, encountered French prehistorians formed in a tradition dominated by Quaternary geology and paleontology (Sackett, 1991). The two groups had such different ideas about the questions to be asked of the archaeological record, and the methods for answering them, that Kuhn's notion of incommensurable paradigms readily comes to mind. And despite decades of subsequent mutual influence and fruitful research collaboration, paradigm-like contrasts are still

apparent. Consider, for example, the discussion by Dibble and Debénath (1991) of their collaborative Paleolithic research in France. United by a shared rigorous field methodology, each was free to pursue his own research agenda, tolerating that of his colleague if not agreeing to its importance. However, even this situation is not a classic Kuhnian paradigm crisis, for what eventually emerged after the Bordes-Binford fire-works was less a paradigm succession, or even a new synthesis, than peaceful coexistence.

If RAO and MRE are different paradigms, they are low-level ones; "subparadigms" might be a suitable term. Substituting subparadigms for Kuhn's paradigms, we can ask whether RAO and MRE differ at this level. Kuhn (1970a) thinks that in some cases the clashes he envisions affect very few scholars in narrow, specialized subdisciplines of a science. So perhaps these are different subparadigms confined to the specialized discipline of paleoanthropology. Yet there is another difficulty to consider. Is it reasonable to portray a debate which has been in progress for over a century as a paradigm clash?

Kuhn (1970a) tells us that paradigms can coexist peacefully, but rarely. Furthermore, "[v]ery few [puzzles] can be traced back to the historic beginning of the science within which they now occur" (1970a:140–141). With a new paradigm, "the whole network of fact and theory that the textbook paradigm fits to nature has shifted" (1970a:141). One could argue that with the discovery of new fossils, theory changes along with fact. But suggesting that every new decade of fossil discoveries results in a paradigm shift is more consonant with journalistic hyperbole than scientific practice. We typically incorporate these new finds within existing theory, though certainly evolutionary theory's articulation with the details of evolutionary history is refined.

In passing, Watkins (1970:34) mentions a criticism of Kuhn's description of scientific progression that he does not pursue but that has relevance here:

One way of challenging it would be to point to historical counterexamples, that is, to long stretches of scientific

history in which no clear paradigm emerged and during which the typical symptoms of Normal Science were absent. I remember Popper saying . . . that, although Newtonianism did turn into something like a paradigm in Kuhn's sense, no such paradigm emerged during the long history of the theory of *matter*. . . [F]rom the pre-Socratics to the present day there has been an unending *debate* between discontinuous and continuous concepts of matter; between various atomic theories on the one hand, and ether, wave and field theories on the other.

Kuhn replies that "a theory of matter is not the sort of topic on which the members of even a single community must necessarily agree" (1970a:180)⁷ and provides a clarification on the scale of paradigm change:

A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than twenty-five people. It is just because this type of change, little recognized or discussed in the literature of the philosophy of science, occurs so regularly on this smaller scale that revolutionary, as against cumulative, change so badly needs to be understood [Kuhn, 1970a:180–181].

These passages suggest that both the disagreements among the philosophers and those within evolutionary biology may stem from the same source, namely simultaneously occurring opposing tendencies of thought with a historical tenacity that transcends current debates.

These tendencies of thought may be labelled discontinuity and continuity, following the description in Watkins' passage cited above. Mazumdar (1995; reviewed by Lesch, 1996) has surveyed the history of immunology and reached a similar conclusion:

In each generation the specific problems and research methods are different, but in Mazumdar's account each conflict expresses a constant set of underlying dichotomies. Whatever the particular issues at stake, one side may be seen to favor unity and continuity, the other plurality and sharply defined difference [Lesch, 1996: 75].

In paleoanthropology, and evolutionary research more generally, Lahr (1996) has en-

⁷His more complete reply is the following: Theories of matter were not, at least until about 1920, the special province or the subject matter for any scientific community. Instead, they were tools for a large number of specialists' groups. Members of different communities sometimes chose different tools and criticized the choice made by others. Even more important, a theory of matter is not the sort of topic on which the members of even a single community must necessarily agree. The need for agreement depends on what it is the community does.

titled opposing paradigms “continuity” and “discontinuity.”

The debate between punctuated equilibrium and gradualism, and the debate in the previous century between catastrophism and uniformitarianism, can be viewed as instances of these two tendencies of thought (see also Brace, 1988). These competing strains also affect species concepts, influencing whether one views species as classes or individuals (Eldredge, 1993; Hull, 1989b; Ruse, 1988).

One commentator on Kuhn not yet mentioned is Stephen Toulmin. His contribution is relevant here not only because he discusses 19th-century paleontological science, but also because he is what one might refer to as a gradualist philosopher. Toulmin (1970) uses paleontology as an analogy to help demonstrate to philosophers of science why Kuhn's proposal of scientific revolutions is not an adequate description of scientific change. Uniformitarians in time accepted that some geologic events (e.g., earthquakes) are capable of producing great and sudden changes, and catastrophists multiplied catastrophes to the point that individual ones necessarily became less catastrophic. Just so, in the philosophy of science,

Once we acknowledge that *no* conceptual change in science is ever absolute, we are left only with a sequence of greater and lesser conceptual modifications differing from one another in degree. The distinctive element in Kuhn's theory is thus destroyed, and we are left looking beyond it for a new sort of theory of scientific change. This theory will have to go beyond both Kuhn's concept of 'revolutions' and the naive uniformitarian views which he renounced, just as Darwin's evolutionary reinterpretation of palaeontology went beyond both the catastrophism of Cuvier and the uniformitarianism of Lyell [Toulmin, 1970:45].

There are theoretical variants in science, just as there are biological variations among organisms in nature; Toulmin (1970:47) prefers an “‘evolutionary’ theory of scientific change” to “Kuhn's ‘catastrophism.’” For Toulmin, “revolution” is not a helpful concept in evolutionary theory, philosophy of science, or political history. Kuhn himself came to modify his views of the nature of scientific revolutions, admitting that the metaphor of a gestalt switch suggests too dramatic a change, at least for a scientific

community as a whole (Hoyningen-Huene, 1993).

Toulmin's evolutionary analogy for scientific change entails difficulties if employed in more than an heuristic sense; for example, science is progressive in a manner biological evolution is not (Ruse, 1986). However, three prominent philosophers of biological science—Peter Bowler, David Hull, and Michael Ruse—agree that the “Darwinian Paradigm” or “Darwinian Revolution” was not the outcome of a Kuhnian scientific event, at least in all the usual senses of these phrases. Hull (1989a) notes that leading philosophers of Darwin's time held essentialist and teleological concepts incompatible with the acceptance of Darwinian evolutionary theory. Bowler places Darwinian theory in the context of competing 19th-century models of developmental evolution, discerning “a continuous process of conceptual development” (1988:198) from the mid-1800s to the modern synthesis; Darwin's *Origin* served the role of a “catalyst” in this transition within biological science. Ruse comments that “there is no question at all but that Darwin's revolution was continuous” (1988:6).

Thus, not only has Kuhn's description of scientific change been questioned, but one point of contention centers around the divergent approaches to the inherent nature of the process of change in any realm. It is possible to see both continuity and discontinuity, depending on the scale selected. One can emphasize either unity or diversity, connection or distinction, depending upon one's point of view and what units are chosen for comparison.

CONCLUSIONS

The reasons for the continuing dispute over modern human origins are complex. The impasse is not simply due to the imperfections of the fossil record and its dating, nor is it just due to one side's refusal to recognize that it is wrong.

Another factor is that of methodological problems—e.g., inconsistent definition and characterization of variables studied, uneven sampling of ancient and modern populations, and failure to distinguish consistently between homologies and other characters in analyses.

Finally, there are theoretical differences. To address the question posed in this article's title, we think that these differences, though important, do not constitute a classic Kuhnian paradigm crisis. Kuhn did not make a conclusion on this question simple to reach, for he used the term "paradigm" in a number of senses. Certainly, though, the AMH issue is significantly unlike the well-known examples of paradigm crisis that Kuhn himself used most (and that most of us associate with the term), such as the Ptolemaic-Copernican struggle. We think so for two main reasons.

First, the conceptual gulf between the two main camps in the AMH origins dispute is considerable, but not so profound as to involve the "incommensurability" associated with most readings of the notion of clashing paradigms. We see the dispute as involving subparadigms within evolutionary biology's overall paradigm of the "modern synthesis" of Darwinism and genetics. Both RAO and MRE proponents accept the same essential evolutionary forces, overall primate phylogeny, and commitment to hypothesis-testing. They do differ strongly on the relative importance in the evolution of modern humanity of various factors—isolation, natural selection, gene flow, and migration—and how these are manifested in the fossil record. These disagreements echo more general problems in evolutionary biology, as over cladogenesis vs. anagenesis and punctuated equilibrium vs. gradualism. The existence of intermediate positions between the RAO and MRE extremes, exemplified by Bräuer and Smith, is a further indication that this dispute is not a clash between incommensurable paradigms, but rather reflects a potential spectrum of views between two poles.

Secondly, we eschew the term "paradigm crisis" to describe the AMH origins dispute because it does not exemplify the classic picture of a period of normal science under one paradigm, giving way to a paradigm crisis, and then the resumption of normal science under a new paradigm. Instead, as we have noted above, the current struggle is the latest manifestation of a long-term conflict over how common cladogenesis and speciation are in human evolution. Although

Kuhn allowed that such disputes could rise to the level of paradigm clashes, he did not pay much attention to them, and it seems to us that the same term should not be applied to both scientific battles with clear winners, and long-term recurring struggles between persistent intellectual perspectives.

Is this subparadigmatic struggle doomed to continue into the indefinite future? We doubt that it will be settled quickly. Along with the database and theoretical difficulties that we have detailed above, we must contend with the strong feelings and entrenched positions now characterizing the two camps.

However, there are several reasons for cautious optimism. Continuing progress in chronology and fossil recovery can be expected, and may eventually resolve important empirical questions—for instance, whether Neanderthals and *Skhül-Qafzeh*-type populations coexisted or alternated in their occupation of the Levant. Similarly, contributions in Paleolithic archaeology and DNA studies will doubtless have important implications for the AMH origins issue. In human paleontology itself, the best avenue of progress probably lies in careful attention, as Willermet and Clark (1995) have urged, to the inferential basis for claims about the emergence of AMH. If the two camps really held incommensurable paradigms, this effort would be useless, but our assessment is more sanguine. Work has begun on such basic issues as the role of developmental plasticity in certain skeletal traits, and reliable differentiation of synapomorphies from other characters in systematic studies of appropriate fossil and modern skeletal samples (e.g., Lahr, 1994, 1996; Lieberman, 1995, 1996; Crummett, 1994; Caspari, 1991). One could also cite Churchill's (1996) recent study indicating that skeletal traits can indeed be phylogenetically and functionally significant in themselves, rather than simply intercorrelated responses to change in overall body form. There is a long way to go, but this rigorous path offers the best hope of reliable reconstruction of modern human evolution and resolution of the current impasse.

ACKNOWLEDGMENTS

We thank Richard Klein, Harry Reeder, and Ronald Wetherington for careful readings and helpful comments on drafts of this paper. The thoughtful suggestions for revision of Ted Steegmann and the *Yearbook* reviewers are likewise much appreciated.

LITERATURE CITED

- Aitken MJ, and Valladas H (1993) Luminescence dating relevant to human origins. In MJ Aitken, CB Stringer, and PA Mellars (eds.): *The Origins of Modern Humans and the Impact of Chronometric Dating*. Princeton: Princeton University Press, pp. 27–39.
- Arambourg C (1958) Les stades évolutifs de l'humanité. *The Leech* 28:106–111.
- Ayala FJ (1995) The myth of Eve: Molecular biology and human origins. *Science* 270:1930–1936.
- Bowler PJ (1988) *The Non-Darwinian Revolution*. Reinterpreting a Historical Myth. Baltimore: Johns Hopkins University Press.
- Brace CL (1964) The fate of the "classic" neanderthals: A consideration of hominid catastrophism. *Curr. Anthropol.* 4:3–43.
- Brace CL (1979) Krapina, "classic" neanderthals, and the evolution of the European face. *J. Hum. Evol.* 8:527–550.
- Brace CL (1981) Tales of the phylogenetic woods: The evolution and significance of phylogenetic trees. *Am. J. Phys. Anthropol.* 56:411–429.
- Brace CL (1988) Punctuationalism, cladistics and the legacy of Medieval neoplatonism. *Hum. Evol.* 3:121–138.
- Brace CL (1995a) *The Stages of Human Evolution*, 5th ed. Englewood Cliffs, NJ: Prentice Hall.
- Brace CL (1995b) Biocultural interaction and the mechanism of mosaic evolution in the emergence of "modern" morphology. *Am. Anthropol.* 97:711–721.
- Bräuer G (1989) The evolution of modern humans: A comparison of the African and non-African evidence. In P Mellars and C Stringer (eds.): *The Human Revolution. Behavioural and Biological Perspectives on the Origins of Modern Humans*. Edinburgh: Edinburgh University Press, pp. 123–154.
- Bräuer G (1992) Africa's place in the evolution of *Homo sapiens*. In G Bräuer and FH Smith (eds.): *Continuity or Replacement. Controversies in Homo sapiens Evolution*. Brookfield, VT: A. A. Balkema, pp. 83–98.
- Brooks AS, Helgren DM, Cramer JS, Franklin A, Hornyak W, Keating JM, Klein RG, Rink WJ, Schwarcz H, Leith Smith JN, Stewart K, Todd NE, Verniers J, and Yellen JE (1995) Dating and context of three Middle Stone Age sites with bone points in the Upper Semliki Valley, Zaire. *Science* 268:548–553.
- Brose DS, and Wolpoff MH (1971) Early Upper Paleolithic man and Late Middle Paleolithic tools. *Am. Anthropol.* 73:1156–1194.
- Brown HI (1977) *Perception, Theory and Commitment. The New Philosophy of Science*. Chicago: University of Chicago Press.
- Cann RL, Rickards O, and Lum JK (1994) Mitochondrial DNA and human evolution: Our one lucky mother. In MH Nitecki and DV Nitecki (eds.): *Origins of Anatomically Modern Humans*. New York: Plenum, pp. 135–148.
- Cartmill M (1990) Human uniqueness and theoretical content in paleoanthropology. *Int. J. Primatol.* 11:173–192.
- Caspari RE (1991) *The Evolution of the Posterior Cranial Vault in the Central European Upper Pleistocene*. Ph.D. dissertation, University of Michigan. Ann Arbor: University Microfilms.
- Cavalli-Sforza LL, Piazza A, Menozzi P, and Mountain J (1988) Reconstruction of human evolution: Bringing together genetic, archaeological, and linguistic data. *Proc. Natl. Acad. Sci. U.S.A.* 85:6002–6006.
- Cavalli-Sforza LL, Menozzi P, and Piazza A (1994) *The History and Geography of Human Genes*. Princeton: Princeton University Press.
- Charlesworth B, Lande R, and Slatkin M (1982) A neo-Darwinian commentary on macroevolution. *Evolution* 36:474–498.
- Churchill SE (1996) Particulate versus integrated evolution of the upper body in Late Pleistocene humans: A test of two models. *Am. J. Phys. Anthropol.* 100:559–583.
- Coon CS (1962) *The Origin of Races*. New York: Alfred A. Knopf.
- Crummett TL (1994) *The Evolution of Shovel Shaping: Regional and Temporal Variation in Human Incisor Morphology*. Ph.D. dissertation, University of Michigan. Ann Arbor: University Microfilms.
- D'Andrade R, and Morin PA (1996) Chimpanzee and human mitochondrial DNA: A principle components and individual-by-site analysis. *Am. Anthropol.* 98:352–370.
- Deacon HJ (1989) Late Pleistocene palaeoecology and archaeology in the Southern Cape, South Africa. In P Mellars and C Stringer (eds.): *The Human Revolution. Behavioural and Biological Perspectives on the Origins of Modern Humans*. Edinburgh: Edinburgh University Press, pp. 547–564.
- Deacon HJ (1993) Southern Africa and modern human origins. In MJ Aitken, CB Stringer, and PA Mellars (eds.): *The Origins of Modern Humans and the Impact of Chronometric Dating*. Princeton: Princeton University Press, pp. 104–117.
- Dibble HJ, and Debénath A (1991) Paradigmatic differences in a collaborative research project. In GA Clark (ed.): *Perspectives on the Past. Theoretical Biases in Mediterranean Hunter-Gatherer Research*. Philadelphia: University of Pennsylvania Press, pp. 217–226.
- Dobzhansky T (1944) On species and races of living and fossil man. *Am. J. Phys. Anthropol.* 2:251–265.
- Eldredge N (1993) What, if anything, is a species? In WH Kimbel and LB Martin (ed.): *Species, Species Concepts, and Primate Evolution*. New York: Plenum, pp. 3–20.
- Foley R (1987) *Another Unique Species. Patterns in Human Evolutionary Ecology*. Essex, England: Longman.
- Frayser DW (1992) Evolution at the European edge: Neanderthal and Upper Paleolithic relationships. *Pré-histoire Européenne* 2:9–69.
- Frayser DW, Wolpoff MH, Thorne AG, Smith FH, and Pope GG (1993) Theories of modern human origins: The paleontological test. *Am. Anthropol.* 95:14–50.
- Frayser DW, Wolpoff MH, Thorne AG, Smith FH, and Pope GG (1994) Getting it straight. *Am. Anthropol.* 96:424–438.
- Gould SJ (1980) Is a new and general theory of evolution emerging? *Paleobiology* 6:119–130.
- Gould SJ, and Eldredge N (1977) Punctuated equilibria: The tempo and mode of evolution reconsidered. *Paleobiology* 3:115–151.
- Gould SJ, and Eldredge N (1993) Punctuated equilibrium comes of age. *Nature* 366:223–227.
- Harrold FB (1991) The elephant and the blind men: Paradigms, data gaps, and the Middle-Upper Paleolithic transition in Southwestern France. In GA Clark (ed.): *Perspectives on the Past: Theoretical Biases in*

- Mediterranean Hunter-Gatherer Research. Philadelphia: University of Pennsylvania Press, pp. 164–182.
- Hennig W (1966) Phylogenetic Systematics. Chicago: University of Illinois Press.
- Hooton EA (1946) Up from the Ape, revised ed. New York: Macmillan.
- Horai S, Hayasaka K, Kondo R, Tsugane K, and Takahata N (1995) Recent African origin of modern human beings revealed by complete sequences of hominoid mitochondrial DNAs. *Proc. Natl. Acad. Sci. U.S.A.* 92:532–536.
- Howell FC (1952) Pleistocene glacial ecology and the evolution of "classic Neandertal" man. *Southwestern J. Anthropol.* 8:377–410.
- Howell FC (1957) The evolutionary significance of variation and varieties of "Neandertal" man. *Q. Rev. Biol.* 32:330–347.
- Howell FC (1994) A chronostratigraphic and taxonomic framework of the origins of modern humans. In MH Nitecki and DV Nitecki (eds.): *Origins of Anatomically Modern Humans*. New York: Plenum, pp. 253–319.
- Howells WW (1944) *Mankind So Far*. Garden City, NY: Doubleday.
- Howells WW (1993) *Getting Here. The Story of Human Evolution*. Washington, DC: Compass.
- Hoyningen-Huene P (1993) *Reconstructing Scientific Revolutions*. Thomas S. Kuhn's Philosophy of Science. Translated by AT Levine. Chicago: University of Chicago Press.
- Hrdlička A (1927) The Neandertal phase of man. *J. R. Anthropol. Inst. G. B. Ire.* 57:249–274.
- Hublin J-J, Barroso Ruiz C, Medina Lara P, Fontugne M, and Reyss J-L (1995) The Mousterian site of Zafarraya (Andalucia, Spain): Dating and implications on the Paleolithic peopling processes of Western Europe. *C. R. Acad. Sci. [IIa]* 321:931–937.
- Hull DL (1989a) Charles Darwin and nineteenth-century philosophies of science. In: *The Metaphysics of Evolution*. Albany: SUNY Press, pp. 27–42.
- Hull DL (1989b) The ontological status of species as evolutionary units. In: *The Metaphysics of Evolution*. Albany: SUNY Press, pp. 79–109.
- Jolly CJ (1993) Species, subspecies, and baboon systematics. In WH Kimbel and LB Martin (eds.): *Species, Species Concepts, and Primate Evolution*. New York: Plenum, pp. 67–107.
- Keith A (1915) *The Antiquity of Man*. London: Williams and Norgate.
- Keith A (1949) Essay XXVI, A new conception of the genesis of modern races. In: *A New Theory of Human Evolution*. New York: Philosophical Library, pp. 256–266.
- Klein RG (1989) *The Human Career. Human Biological and Cultural Origins*. Chicago: University of Chicago Press.
- Klein RG (1994) The problem of modern human origins. In MH Nitecki and DV Nitecki (eds.): *Origins of Anatomically Modern Humans*. New York: Plenum, pp. 3–21.
- Klein RG (1995) Anatomy, behavior, and modern human origins. *J. World Prehist.* 9:167–198.
- Kuhn TS (1970a) *The Structure of Scientific Revolutions*, 2nd ed., Enlarged. Chicago: University of Chicago Press.
- Kuhn TS (1970b) Logic of discovery or psychology of research? In I Lakatos and A Musgrave (eds.): *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, London, 1965, Volume 4. New York: Cambridge University Press, pp. 1–23.
- Kuhn TS (1970c) Reflections on my critics. In I Lakatos and A Musgrave (eds.): *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, London, 1965, Volume 4. New York: Cambridge University Press, pp. 231–278.
- Lahr MM (1994) The multiregional model of modern human origins: A reassessment of its morphological basis. *J. Hum. Evol.* 26:23–56.
- Lahr MM (1996) *The Evolution of Modern Human Diversity. A Study of Cranial Variation*. New York: Cambridge University Press.
- Lahr MM, and Foley R (1994) Multiple dispersals and modern human origins. *Evol. Anthropol.* 3:48–60.
- Lakatos I (1970) Falsification and the methodology of scientific research programmes. In I Lakatos and A Musgrave (eds.): *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, London, 1965, Volume 4. New York: Cambridge University Press, pp. 91–196.
- Lesch JE (1996) Immunology dichotomized. (Review of Mazumdar; see citation below.) *Science* 273:75–76.
- Lieberman DE (1995) Testing hypotheses about recent human evolution from skulls. Integrating morphology, function, development, and phylogeny. *Curr. Anthropol.* 36:159–197.
- Lieberman DE (1996) How and why humans grow thin skulls: Experimental evidence for systemic cortical robusticity. *Am. J. Phys. Anthropol.* 101:217–236.
- Lieberman DE, and Shea JJ (1994) Behavioral differences between archaic and modern humans in the Levantine Mousterian. *Am. Anthropol.* 96:300–332.
- Livingstone FB (1992) Gene flow in the Pleistocene. *Hum. Biol.* 64:67–80.
- Livingstone FB (1996) Genes don't flow. *Am. J. Phys. Anthropol.* [Suppl.] 22:151.
- Masterman M (1970) The nature of a paradigm. In I Lakatos and A Musgrave (eds.): *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, London, 1965, Volume 4. New York: Cambridge University Press, pp. 59–89.
- Mayr E (1963) *Animal Species and Evolution*. Cambridge: Harvard University Press.
- Mazumdar PMH (1995) *Species and Specificity. An Interpretation of the History of Immunology*. New York: Cambridge University Press.
- Penny D, Watson E, and Steel M (1993) Trees from languages and genes are very similar. *Syst. Biol.* 42:382–384.
- Popper KR (1959) *The Logic of Scientific Discovery*. New York: Basic Books.
- Popper KR (1970) Normal science and its dangers. In I Lakatos and A Musgrave (eds.): *Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science*, London, 1965, Volume 4. New York: Cambridge University Press, pp. 51–58.
- Popper KR (1974a) *Autobiography of Karl Popper*. In PA Schilpp (ed.): *The Philosophy of Karl Popper. The Library of Living Philosophers, Volume XIV, Book I, Part I*. La Salle, IL: Open Court, pp. 1–181.
- Popper KR (1974b) *Replies to my critics*. In PA Schilpp (ed.): *The Philosophy of Karl Popper. The Library of Living Philosophers, Volume XIV, Book II, Part III*. La Salle, IL: Open Court, pp. 959–1197.
- Popper KR (1985) *Natural selection and its scientific status* (1977). In D Miller (ed.): *Popper Selections*. Princeton: Princeton University Press, pp. 239–246.
- Relethford JH, and Harpending HC (1995) Ancient differences in population size can mimic a recent African origin of modern humans. *Curr. Anthropol.* 36:667–676.
- Rogers AR, and Jorde LB (1995) Genetic evidence on modern human origins. *Hum. Biol.* 67:1–36.

- Ruse M (1986) Taking Darwin Seriously. A Naturalistic Approach to Philosophy. New York: Basil Blackwell.
- Ruse M (1988) Philosophy of Biology Today. Albany: SUNY Press.
- Ruse M (1989a) What kind of revolution occurred in geology? In: The Darwinian Paradigm. Essays on its History, Philosophy, and Religious Implications. New York: Routledge, pp. 55–89.
- Ruse M (1989b) Is the theory of punctuated equilibria a new paradigm? In: The Darwinian Paradigm. Essays on its History, Philosophy, and Religious Implications. New York: Routledge, pp. 118–145.
- Sackett JR (1991) Straight archaeology French style: The phylogenetic paradigm in historic perspective. In GA Clark (ed.): Perspectives on the Past. Theoretical Biases in Mediterranean Hunter-Gatherer Research. Philadelphia: University of Pennsylvania Press, pp. 109–139.
- Schwarz HP, and Grün R (1993) Electron spin resonance (ESR) dating of the origin of modern man. In MJ Aitken, CB Stringer, and PA Mellars (eds.): The Origins of Modern Humans and the Impact of Chronometric Dating. Princeton: Princeton University Press, pp. 40–48.
- Shea JJ (1993) Lithic use-wear evidence for hunting by Neandertals and early modern humans from the Levantine Mousterian. In G Peterkin, H Bricker, and P Mellars (eds.): Hunting and Animal Exploitation in the Later Palaeolithic and Mesolithic of Eurasia. Washington, DC: Archeol. Papers Am. Anthropol. Assoc., No. 4, pp. 189–197.
- Sherry ST, Rogers AR, Harpending H, Soodyall H, Jenkins T, and Stoneking M (1994) Mismatch distributions of mtDNA reveal recent human population expansions. *Hum. Biol.* 66:761–775.
- Shreeve J (1995) The Neandertal Enigma. Solving the Mystery of Human Origins. New York: William Morrow.
- Simpson GG (1961) Principles of Animal Taxonomy. New York: Columbia University Press.
- Smith FH (1991) The neandertals: Evolutionary dead ends or ancestors of modern people? *J. Anthropol. Res.* 47:219–238.
- Smith FH (1992) The role of continuity in modern human origins. In G Bräuer and FH Smith (eds.): Continuity or Replacement. Controversies in *Homo sapiens* Evolution. Brookfield, VT: A. A. Balkema, pp. 145–156.
- Smith FH, and Paquette SP (1989) The adaptive basis of neandertal facial form, with some thoughts on the nature of modern human origins. In E Trinkaus (ed.): The Emergence of Modern Humans. Biocultural Adaptations in the Later Pleistocene. New York: Cambridge University Press, pp. 181–210.
- Spencer F (1979) Aleš Hrdlička, M.D., 1869–1943. A Chronicle of the Life and Work of an American Physical Anthropologist. Ph.D. dissertation, University of Michigan. Ann Arbor: University Microfilms.
- Spencer F (1984) The neandertals and their evolutionary significance: A brief historical survey. In FH Smith and F Spencer (eds.): The Origins of Modern Humans: A World Survey of the Fossil Evidence. New York: Alan R. Liss, pp. 1–49.
- Spencer F, and Smith FH (1981) The significance of Aleš Hrdlička's "Neanderthal phase of man": A historical and current assessment. *Am. J. Phys. Anthropol.* 56:435–459.
- Straus WL, Jr, and Cave AJE (1957) Pathology and the posture of Neanderthal man. *Q. Rev. Biol.* 32:348–363.
- Stringer CB (1974) Population relationships of Later Pleistocene hominids: A multivariate study of available crania. *J. Archaeol. Sci.* 1:317–342.
- Stringer CB (1982) Towards a solution to the neanderthal problem. *J. Hum. Evol.* 11:431–438.
- Stringer CB (1989a) Documenting the origin of modern humans. In E Trinkaus (ed.): The Emergence of Modern Humans. Biocultural Adaptations in the Later Pleistocene. New York: Cambridge University Press, pp. 67–96.
- Stringer CB (1989b) The origin of early modern humans: A comparison of the European and non-European evidence. In P Mellars and C Stringer (eds.): The Human Revolution. Behavioural and Biological Perspectives on the Origins of Modern Humans. Edinburgh: Edinburgh University Press, pp. 232–244.
- Stringer CB (1992) Replacement, continuity and the origin of *Homo sapiens*. In G Bräuer and FH Smith (eds.): Continuity or Replacement. Controversies in *Homo sapiens* Evolution. Brookfield, VT: A.A. Balkema, pp. 9–24.
- Stringer CB (1993) Reconstructing recent human evolution. In MJ Aitken, CB Stringer, and PA Mellars (eds.): The Origins of Modern Humans and the Impact of Chronometric Dating. Princeton: Princeton University Press, pp. 179–195.
- Stringer CB (1994) Out of Africa—A personal history. In MH Nitecki and DV Nitecki (eds.): Origins of Anatomically Modern Humans. New York: Plenum, pp. 149–172.
- Stringer CB (1995) The evolution and dispersal of Later Pleistocene human populations. In E Vrba, GH Denton, TC Partridge, and LH Burckle (eds.): Paleoclimate and Evolution with Special Reference to Human Origins. New Haven: Yale University Press, pp. 524–531.
- Stringer CB, and Andrews P (1988a) Genetic and fossil evidence for the origin of modern humans. *Science* 239:1263–1268.
- Stringer C, and Andrews P (1988b) Response (to Wolpoff et al. letter). *Science* 241:773–774.
- Stringer C, and Bräuer G (1994) Methods, misreading, and bias. *Am. Anthropol.* 96:416–424.
- Stringer C, and Gamble C (1993) In Search of the Neanderthals. Solving the Puzzle of Human Origins. New York: Thames and Hudson.
- Stringer C, and McKie R (1996) African Exodus. London: Jonathan Cape.
- Templeton AR (1993) The "Eve" hypotheses: A genetic critique and reanalysis. *Am. Anthropol.* 95:51–72.
- Toulmin S (1970) Does the distinction between normal and revolutionary science hold water? In I Lakatos and A Musgrave (eds.): Criticism and the Growth of Knowledge. Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Volume 4. New York: Cambridge University Press, pp. 39–47.
- Trinkaus E (1976) The morphology of the European and Southwest Asian neandertal pubic bones. *Am. J. Phys. Anthropol.* 44:95–104.
- Trinkaus E (1977) A functional interpretation of the axillary border of the neandertal scapula. *J. Hum. Evol.* 6:231–234.
- Trinkaus E (1983) The Shanidar Neandertals. New York: Academic.
- Trinkaus E, and Howells WW (1979) The neanderthals. *Sci. Am.* 241:118–133.
- Trinkaus E, and Shipman P (1992) The Neandertals. Of Skeletons, Scientists, and Scandal. New York: Vintage.
- Walker A, and Shipman P (1996) The Wisdom of the Bones. In Search of Human Origins. New York: Alfred A. Knopf.
- Watkins J (1970) Against 'normal science.' In I Lakatos and A Musgrave (eds.): Criticism and the Growth of Knowledge. Proceedings of the International Collo-

- quium in the Philosophy of Science, London, 1965, Volume 4. New York: Cambridge University Press, pp. 25–37.
- Weidenreich F (1940) Some problems dealing with ancient man. *Am. Anthropol.* 42:375–383.
- Weidenreich F (1943) The "Neanderthal man" and the ancestors of "*Homo sapiens*." *Am. Anthropol.* 45:39–48.
- Weidenreich F (1946) Generic, specific and subspecific characters in human evolution. *Am. J. Phys. Anthropol.* 4:413–432.
- Weidenreich F (1947) Facts and speculations concerning the origin of *Homo sapiens*. *Am. Anthropol.* 49:187–203.
- Willermet CM (1993) The Debate over Modern Human Origins: A Scientific Tug-of-War. Unpublished M.A. thesis, Arizona State University.
- Willermet CM, and Clark GA (1995) Paradigm crisis in modern human origins research. *J. Hum. Evol.* 29:487–490.
- Wisdom JO (1974) The nature of 'normal' science. In PA Schilpp (ed.): *The Philosophy of Karl Popper*. The Library of Living Philosophers, Volume XIV, Book II, Part II. La Salle, IL: Open Court, pp. 820–842.
- Wolpoff MH (1989a) The place of the neandertals in human evolution. In E Trinkaus (ed.): *The Emergence of Modern Humans. Biocultural Adaptations in the Later Pleistocene*. New York: Cambridge University Press, pp. 97–141.
- Wolpoff MH (1989b) Multiregional evolution: The fossil alternative to Eden. In P Mellars and C Stringer (eds.): *The Human Revolution. Behavioural and Biological Perspectives on the Origins of Modern Humans*. Edinburgh: Edinburgh University Press, pp. 62–108.
- Wolpoff MH (1992) Theories of modern human origins. In G Bräuer and FH Smith (eds.): *Continuity or Replacement. Controversies in Homo sapiens Evolution*. Brookfield, VT: A. A. Balkema, pp. 25–63.
- Wolpoff MH (1996) *Human Evolution*. New York: McGraw-Hill.
- Wolpoff MH, and Caspari R (1996) The modernity mess. *J. Hum. Evol.* 30:167–172.
- Wolpoff MH, Xinzhi Wu, and Thorne AG (1984) Modern *Homo sapiens* origins: A general theory of hominid evolution involving the fossil evidence from East Asia. In FH Smith and F Spencer (eds.): *The Origins of Modern Humans. A World Survey of the Fossil Evidence*. New York: Alan R. Liss, pp. 411–483.
- Wolpoff MH, Spuhler JN, Smith FH, Radović J, Pope G, Frayer DW, Eckhardt R, and Clark G (1988) Modern human origins. *Science* 241:772–773.
- Wolpoff MH, Thorne AG, Smith FH, Frayer DW, and Pope GG (1994a) Multiregional evolution: A worldwide source for modern human populations. In MH Nitecki and DV Nitecki (eds.): *Origins of Anatomically Modern Humans*. New York: Plenum, pp. 175–199.
- Wolpoff MH, Thorne AG, Jelinek J, and Zhang Yinyun (1994b) The case for sinking *Homo erectus*. 100 years of *Pithecanthropus* is enough! *Cour. Forsch. Inst. Senckenberg* 171:341–361.
- Wood B (1994) The problems of our origins. *J. Hum. Evol.* 27:519–529.
- Yellen JE, Brooks AS, Cornelissen E, Mehlman MJ, and Stewart K (1995) A Middle Stone Age worked bone industry from Katanda, Upper Semliki Valley, Zaire. *Science* 268:553–556.